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Vegetable parasites and evolution.[1]

W. G FARLOW.

In the countless discussions concerning evolution which have followed the publication of Darwin's Origin of Species, zoölogists have gone farther than botanists in their efforts to explain the possible origin of higher forms from the lower. Botanists, as a rule, have contented themselves with a consideration of the ancestral relations of the orders of higher plants, but, until very recently, they have scarcely made any serious attempt to present a general scheme showing, from an evolutionary point of view, the relations of all the groups of the vegetable kingdom. This may be due either to their timidity—perhaps modesty is a better sounding word—or to their ignorance. If the latter, they have certainly been wise in avoiding unnecessary display of their ignorance; if the former, they can easily be pardoned, when one considers how large a part an aggressive audacity savoring of sensationalism has played in the formation of some schemes of development.

On abstract grounds alone, I presume that few botanists would object to the statement that all plants have been developed from simple ancestral forms, which were nearly related to some of the lower animals; but, when it comes to saying in anything like a definite way that certain existing forms have arisen from other lower existing forms or their immediate allies in some past epoch, and so on until the lowest form is reached, botanists may well insist that imagination should not be allowed too large a scope in supplying missing links. It is precisely in this point that zöologists have an advantage over botanists. The palæontological record of lower animals is more complete than that of lower plants, so that, where the zoölogist might reasonably form an hypothesis, the botanist must rely more on his imagination, until, in the end, he finds himself in the possession of a chain composed to a considerable extent of missing links. As it is, if we would consider the evolution of plants, not getting much light on the progress of the lower forms from palæontology, we are compelled to trust largely to plants as we now

[1] Vice-Presidential address before Section F, A. A. A. S., New York, August 10, 1887.

find them, and to ask what are the inferences which we are permitted to draw from existing structures and conditions. I shall not attempt to offer any scheme of development, or to sketch a family tree whose roots are Protococci and bacteria and whose ripe apples are the genera of Phænogams, but shall restrict myself to some considerations concerning vegetable parasites and the inferences as to their possible origin which may be gathered from what we already know of their structure and habits; partly because this is a group of plants in which I am especially interested, and partly because the problems which they offer, even if they can not be solved at the present day, are, at least, full of suggestions.

In the first place, a word as to the different kinds of plants which are included among parasites. A parasite is usually defined as a plant which is unable to transform inorganic material into organic compounds, and which is consequently obliged to obtain its organized materials from other plants or from animals. The definition, in general, is an accurate one and correctly defines the vast majority of vegetable parasites which belong to the class of fungi. That they are strictly dependent on the organized materials derived eventually from other plants or animals is sufficiently evident when we consider that fungi are destitute of chlorophyll, the necessary agent in the assimilation of inorganic material. Of the parasites proper, we have two kinds: the saprophytes, which live on dead or inert matter; and the special, or true, parasites, as they are usually called, which can only grow on the tissues of living plants or animals. Whether the line between saprophytes and true parasites is sharply defined is a point which need not at present be discussed. It is enough to say that, as a rule, saprophytes grow more or less indiscriminately on dead organic substances, while the true parasites are generally limited to a single species of plants or to the species of a single genus or order. It is almost unnecessary to cite instances of the two kinds of parasites proper, since you will at once call to mind Penicillium and other common moulds which grow as saprophytes on an endless variety of substances; while we have as illustrations of true parasites the grape Peronospora, which grows abundantly on species of Vitis, and is occasionally found on Ampelopsis and Cissus, both genera of Vitaceæ, and the potato-rot fungus, sometimes found on the tomato, which belongs to the same order as the potato, and rarely on species of Scrophulariaceæ, a nearly related order.

The proper parasites do not exclusively belong to the class of fungi. You are familiar with the Indian pipe (Monotropa) and dodder (Cuscuta), which are our common representatives of parasites, which are found in a comparatively small number of orders of Phænogams. As chlorophyll is wanting in these plants, we are forced to assume that the parasitism is as complete as in fungi. You, also, will recall the mistletoes and the Gerardias, together with other members of the Scrophulariaceæ, which are not proper parasites in the sense in which we have already spoken, but may rather be called partial parasites; because, while they have chlorophyll and are to a certain extent able to transform inorganic into organic material, they still depend in part on material taken from other plants to which they are more or less closely attached. In the discussion of the evolution of parasites, the phænogamic parasites, however, are of comparatively little importance; because, by means of their flowers and fruit, they are rightly classed as belonging to, or closely related to, well recognized orders of Phænogams, and the question of the origin of the parasites themselves is not to be separated from the question of the origin of Phænogams as a whole, so that, in this case, we have only to account for the modification of the organs of vegetation whose greater simplicity may be explained by the loss of leaves and other assimilating organs which have become unnecessary to plants that have acquired the power of living upon the food assimilated by other plants. In short, as far as parasitic Phænogams are concerned, they may be regarded as degenerate forms of other Phænogams, for, in a plant, the inability to assimilate inorganic material should be regarded as a degraded condition in which the chances of survival are diminished unless some extraordinary provision is made for reproduction, which is not the case in Phænogams, whatever may be true of fungi.

Whether any proper parasites are to be found among algæ is a question on which there is a difference of opinion. For my own part, I am unable to recognize any proper parasite among algæ, although it is tolerably certain that a number of forms generally classed among algæ may be regarded as partial parasites. This point, however, can be better considered later on.

Let us next briefly consider the mutual relations which exist between parasites and their hosts—that is, the substances, dead or living, on which they are growing. At first sight a parasite would seem to be purely destructive in its action; and that this is really the case is evident in the great majority of instances. When a piece of bread is attacked by the mould, Mucor stolonifer, its substance continually diminishes with the growth of the mould. We need not stop to consider the saprophytes, for the case of the true parasites is still stronger. There is a constant struggle between the rots, rusts and other true parasites and the hosts on which they are growing. Just so far as the fungus flourishes, so does the host suffer; and, if the conditions of temperature and moisture are favorable to the fungus, the host may be quite destroyed. If the conditions are not favorable to the fungus, the host may continue to grow, and the fungus may gradually disappear, or, at least, pass into a quiescent state. An instance of the sudden and complete destruction of the host is seen in the case of the bad epidemics of the potato rot, when whole acres suddenly rot and die. This is an extreme case. Other members of the Peronosporeæ attack young seedlings, some of which are destroyed, while others continue to grow, and may be said to throw off the fungus. From cases of the complete and sudden destruction of the host, either in its mature or seedling condition, we pass to parasites which are less virulent and whose action is more local. We have all grades of injury done to the host, from the destruction of the leaves and consequent diminution of the assimilating power, which may entail serious or fatal results; from the formation of circumscribed knots and tumors, which may cause destruction of the branches and, in course of years, the death of the plant; from fungi which attack the flowers or fruit and cause diminished reproductivity without injury to the vegetative powers, down to the insignificant distortions of scattered epidermal cells caused by Synchytria. But in all these cases the action of the parasite is destructive. We can not conceive that it is of the slightest benefit to the host. It robs the plant of the food which it needs for itself, and gives back nothing good in return.

We have, on the other hand, instances of parasitism in which it is claimed that the relation of parasite and host[2] is

[2] The word "symbiosis" was originally applied to all cases where different organisms were associated together in a community, and in this sense included the true parasites. The application has gradually been modified until, at the present day, symbiosis is generally understood to mean the association of plants with plants, or plants with animals, in such a way that the relation between them is one of mutual benefit, or in which there is, at least, no injurious action of one organism on the other. In this sense, as contrasted with true parasitism, the word is here used.

one of mutual benefit. To this condition the name symbiosis has been applied. The two most marked instances of symbiosis among plants are to be found in lichens and the fungus growth first called by Frank Mycorhiza.

The thallus of lichens, you will bear in mind, is composed of two elements: the green cells or filaments called gonidia, and the colorless threads or hyphæ. Any extended discussion of the algo-fungal theory of lichens would be out of place on the present occasion, and it is only necessary to say that I do not see why we may not consider the gonidia to be what they appear to be, viz., algæ, and the hyphæ fungi parasitic on the gonidia. Certainly, the opponents of the algo-fungal theory, in spite of all their attempts, have not, as yet, given satisfactory proof that the gonidia are produced from the hyphæ or the hyphæ from the gonidia; so that we are forced to regard them as two distinct entities. The strong point of the opponents of the algo-fungal theory has been that, if it is true that what is called a lichen is really a fungus parasitic on an alga, it is inconceivable that the alga should not be injured, or even destroyed, by the fungus. It is certainly a fact that the gonidia, or algæ, are not destroyed, and it has been assumed by both the advocates and opponents of the theory that the gonidia are not injured by the growth of the hyphæ, while some even go so far as to say that their growth is aided thereby. To account for this state of things, the advocates of the theory have advanced the view that in lichens we have a sort of mutual parasitism; and the statement has been made that "the hyphæ lie on the gonidia, and carry to them crude nutritive fluids, in return for which they receive a part of the assimilated material in the gonidia." But what good the gonidia can derive by having crude material brought to them by the hyphæ, if they must give back a part of the assimilated material to them, is not clear, since it is a well-known fact that the gonidia can, and very often do, live and flourish in a free condition, and are amply able to obtain all the nourishment they need without the help of the hyphæ, and at the same time can use for their own exclusive benefit all the assimilated material. On the other hand, it is known that the hyphæ are dependent on the gonidia for their development. The advantage to the gonidia is quite hypothetical; the advantage to the hyphæ is real; and it is, to speak mildly, a bad case of what the French call *un œuf pour un bœuf*.

The alleged proof that the gonidia are benefited by contact with the hyphæ rests on laboratory cultures in which it is claimed that, if the germinating spores of lichens be brought in contact with pure gonidia, the hyphæ at once grow more rapidly, and the gonidia also begin to multiply. But this increase of the gonidia is not necessarily a sign that the conditions of growth have become more favorable. When the black knot fungus attacks a branch of the plum tree the parenchymatous cells increase, and a knot is formed; and the same thing occurs when branches of red cedar are attacked by Gymnosporangium macropus. Here the increased growth does not indicate an increased supply of food, but an irritation caused by a noxious parasite. The increased growth of normal cells in the presence of irritating foreign bodies is well known to both animal and vegetable pathologists, and is not interpreted by them to mean an improved condition, but rather an attempt to get rid of something harmful. The same explanation may be given to the lichen cultures. But cultures on microscopic slides in the laboratory surely should not be regarded as more conclusive than what is seen on a much larger scale in nature. One has only to compare the Chroolepus-forms which constitute the gonidia of Opegrapha with the same forms when free from the hyphæ of the Opegrapha to be convinced that they grow and fructify decidedly better when free than when shut up in the lichen thallus. They are neither benefited nor destroyed, but they are weakened and injured. The same is true of the cystococcoid or protococcoid gonidia of the larger lichens, which are more luxuriant when growing free on rocks and bark. It is impossible to regard the Stigonema gonidia, distorted and broken up by the hyphæ, as in a more flourishing condition than when free.

It seems to me that the real error of the supporters of the algo-fungal theory is not that they assume that the gonidia, the algæ, can support themselves and the hyphæ too, but that they assume that they are not injured thereby. In their attempt to show how a possible advantage to the gonidia might arise, they have not sufficiently regarded the palpably injurious action of the hyphæ. From the facts which I have given it is plain that they are injured, and, if the injury is less than in most cases of parasitism—which may be due to the fact that the hyphæ of lichens grow more slowly than those of other fungi—it is nevertheless an injury, and we must recognize in lichens not a case of symbiosis or mutual parasitism, but a case of true parasitism with a minimum of injury to the host. In view of the facts, one can be an advocate of the algo-fungal theory without believing that there is a double parasitism.

In 1885[3] Frank announced the following discovery: that certain species of trees, especially Cupuliferæ, do not regularly obtain their food directly from the soil, but their roots are connected with the mycelium of a fungus by whose agency all the nourishment is transferred from the soil to the tree. He called this condition Mycorhiza, and described the fungus as intimately united with the inner cortex of the roots just back of the tips and forming a felt-like cap over the tips. He maintained that this union of mycelium and roots was of constant occurrence in the Cupuliferæ which he had examined, oaks, beeches, chestnuts, hazel-nuts and hornbeams, and more or less constant in Salicaceæ and Coniferæ. At a later date[4] he went further and stated that the Mycorhiza is a symbiotic condition which may perhaps be found in all trees under certain conditions; that it is found only where the soil consists of humus or undecomposed plant remains; that the fungus of the Mycorhiza conveys to the tree not only the necessary water and the mineral constituents of the soil, but also the organic material derived directly from the humus and decomposing vegetable matter; and that it is through the agency of the fungus alone that the tree obtains its food from the soil. If one could accept without reserve the conclusions of Frank, we have in Mycorhiza a clear case of symbiosis in which a fungus which lives as a saprophyte on vegetable mould is intimately united with the tissues of Phænogams on which it acts, not as a parasite but as a conveyer of nourishment. Unfortunately, the statements of Frank are, to a great extent, not confirmed by other competent observers. R. Hartig has shown[5] that the Mycorhiza condition is not at all necessary to the nourishment of trees even in Cupuliferæ, since he finds that, in many cases, roots of healthy trees are quite free from Mycorhiza, and, even in trees where there is a marked Mycorhiza of some roots, there are others quite free from it. He regards Mycorhiza not as a case of symbiosis comparable to that of lichens, as does Frank, but rather a case of proper parasitism, and Kamienski[6] states that, in the cases of Mycorhiza of trees which he has examined, he has always found evidences of injury done to the roots by

[3] Ber. Deutsch Bot. Ges. III, 128.
[4] l. c. III, XXVII.
[5] Bot. Centralblatt, XXV, 350.
[6] Bot. Centralblatt, XXX, 2.

the fungus, which he also regards as a parasite of a destructive nature. P. E. Mueller, to a certain extent, endorses Frank's views, as far as the Mycorhiza of beeches is concerned.

We are, on the whole, warranted in believing that the Mycorhiza condition is rather a condition of proper parasitism than of symbiosis in the case of trees. We still have the case of Monotropa Hypopitys, a small ericaceous parasite, in which Kamienski showed, as early as 1881, that the roots are surrounded by a mycelium, which, however, does not penetrate into the substance of the roots, as in the Mycorhiza of Frank. He considered that the fungus, in this case, was the medium of transfer of nourishment to the Monotropa, and did not agree with the then prevailing view that Monotropa itself was directly parasitic on the roots of other plants. We may safely consider that there is a symbiosis in Monotropa Hypopitys, and further investigation may show a similar condition in some other closely related phænogamous plants which are destitute of chlorophyll; but here the case is very different from that of the large trees, abundantly provided with assimilating organs of their own in which, if there is symbiosis at all, it certainly does not exist on the wholesale scale which Frank claims.

With regard to the symbiosis of plants and animals I will say but a word, for the subject is one which pertains to the domain of the zoölogist rather than to that of the botanist. The inherent objections against the probability that plants and animals should live in a state of symbiosis are less than in the case of symbiosis in the vegetable kingdom; because, in the former case, the plants in question belong not to the group of fungi, but are algæ possessing chlorophyll, or a modified form of chlorophyll. The symbiotic alga could ~~not~~ support itself; the animal, on the other hand, could support itself; and, bearing in mind the different products of assimilation and respiration in plants and animals, one could easily conceive the benefit which might arise from the combination of the two. Whether the combination really exists in many cases is not yet certain, because it too frequently happens that zoölogists do not agree as to whether the assumed alga is really an alga or a proper organ of the animal itself.

It becomes a question of authority, and a botanist is not in a condition to estimate the comparative merits of observations made by zoölogists. As far as I am at liberty to form any opinion at all, I should say that zoölogists were inclined to accept, at least, a mechanical symbiosis of unicellular algæ and animals in a considerable number of instances. Whether the symbiosis is physiological as well as mechanical is a point on which more light is apparently needed.

The symbiosis of plants and animals is, perhaps, better to be compared with that of Nostocs with Hepaticæ and Azolla than with the condition which exists in lichens. Some of the recorded cases show clearly a mechanical symbiosis, even if others be regarded as merely accidental and temporary unions of different organisms. Whether the symbiosis here is physiologically of advantage to both organisms is doubtful. The Nostocs are certainly not injured, and they may derive benefit from the shelter afforded. It will not do to go too far in this direction, however, because we should, at length, be forced to speak of symbiosis in cases where Nostocs grow in crevices of rocks, which would be absurd.

I have dwelt somewhat at length on the subject of symbiosis because, as it seems to me, botanists have gone too far in assuming a beneficial action of the parasite on the host in many cases where not only no direct benefit can be proved to exist, but where a closer examination shows that an injury is really done, although it may be slight. In short, symbiosis, as distinct from true destructive parasitism, is not the comparatively common condition in the vegetable kingdom which it is generally supposed to be by those whose opinion is worth considering; for we need not regard those writers who, seeing in symbiosis a charming instance of domestic felicity and concord with which they can point a moral and adorn a tale, have given to the public essays whose only proper place is on the shelves of a Sunday School library. Accepting the existence of symbiosis where both members are chlorophyll-bearing plants, we must still believe that, with rare exceptions, the cases where one member is a fungus should be referred rather to the class of true parasites, in which the advantage is altogether on one side.

If we turn now to the question of the origin of vegetable parasites we find ourselves in a dilemma. Certainly, the parasites could not have originated before the plants and animals on whose remains or in whose tissues they live. On the other hand, accepting the law of evolution, that the more complex forms are developed later than the simpler forms, the parasites must have preceded the forms on which they prey. The paradox is, however, more in words than in reality. We can only suppose that our present parasites have existed from early times, but were not always parasites. The question might arise here, What do we mean by higher and lower forms? The terms are elastic, and one sometimes suspects that they have been stretched and twisted to suit the necessities of individual writers. It is not quite plain, for instance, why we should say that the giant kelp of the Pacific, Macrocystis pyrifera, with its branching stems several hundred feet long, furnished with innumerable leaves and air-bladders, is less highly organized than the small frondose hepatics, like Riccia, or such mosses as Phascum. There is one point on which all botanists would probably agree in speaking of high or low organizations, viz.: that complications of the reproductive apparatus indicate a high organization, however simple the vegetative organs may be, and that as we advance higher in the scale we find more and more numerous embryonic conditions which represent free conditions of less highly organized plants.

Throwing out of consideration the phænogamous parasites for the reasons previously given, there is no doubt that the immense majority of vegetable parasites belong low down in the scale of development, and we can infer from the simplicity of their reproduction that they originated at an early period. Other things also point in the same direction. In the class of fungi, although the sexual reproduction is of low grade, it embraces a number of different types, and, as far as non-sexual modes of propagation are concerned, although it may be said that they only indicate an effort on the part of the plants to adapt themselves to peculiar conditions, fungi are far better provided than any other plants. We are, perhaps, at liberty to suppose a remote origin from the large number of species of fungi now in existence, and, in this connection, a few statistics may prove of interest. The question is frequently asked whether the species of fungi are more numerous than those of Phænogams. It is safe to suppose that they are, although it is not true that more species of fungi have already been described. The systematic study of fungi in Europe and North America is of so much more recent date than the study of the Phænogams of those two continents, while the fungi of a large share of the earth have scarcely been studied at all, that a comparison of the described species of the two classes fails to give a correct estimate of the real numbers. The reason for supposing that the species of fungi are more numerous than those of Phænogams is founded on the fact that, in countries where fungal flora has been most thoroughly studied, we find few species of Phænogams which are not already known to be attacked by some special parasite, while the majority of species serve as hosts for a considerable number of species of fungi. A few figures will show this point clearly. In his treatise on the fungi which attack the species of Vitis, published in 1879, Pirotta enumerates one hundred and four species of parasites. Between ten and twenty of these are fungi not found on Vitis alone, but this number is more than counterbalanced by species peculiar to Vitis which have been described since 1879. It may be objected that some of the forms called species by Pirotta are probably merely stages of some of the other species enumerated. Admitting that this is possible, and even probable, if we deduct half, or even two-thirds, which is liberal to the last degree, we still have thirty to fifty species of fungi, at the lowest estimate, which are peculiar to six species of Vitis, the number of species of the genus included in Pirotta's observations. I have little doubt that the real number of species of fungi peculiar to the genus Vitis is much larger than the estimate I have just given. If the relative number of species known to occur on Vitis is greater than that of those known on most other genera, it is due rather to the fact that, from their importance in horticulture, they have been more carefully studied than because other genera are less frequented by special fungi.

The province of Venetia is probably no richer in fungi than other parts of the world; but, as it is of small size and is the residence of a considerable number of mycologists, its flora has been more thoroughly studied than that of this country, and we can obtain a more accurate view if we examine statistics of the Venetian flora. Cuboni and Mancini enumerate sixty-five species of fungi which occur on the chestnut, and over three hundred species on Quercus, including three native species of that genus. If we deduct a large number for species which are not found exclusively on these two genera or which are merely secondary forms of other species, we still have a considerable number of fungi to a small number of Phænogams. Turning to the American flora, we find that the species of a genus as erratic as Sarracenia are not without their proper parasites; for on four species of Sarracenia we already know four species of fungi, three of which are peculiar to the genus. The list of fungi which grow on oaks in the United States includes between five hundred and six hundred species. The greater part, however, are not peculiar to oaks, and, as the synonymy of the species is much confused, the exact number of fungi known on all our oaks can not be given exactly. On Quercus alba fifty-seven and on Q. tinctoria forty-six species are reported, about a quarter, or possibly a third, of which are probably peculiar to those species.

We can start with the postulate that vegetable parasites must have originated at an early epoch and must have been derived from non-parasitic forms. What forms? Here we enter upon the field of pure speculation. It can hardly be supposed that we shall ever know what was the earliest form of life. It may have been some protoplasmic structure which was neither strictly vegetable nor animal. Probably the earliest forms of undoubted plants were unicellular forms like Protococcus. The term Protococcus, as used at the present day, includes some forms which are claimed by zoölogists; whether rightly or wrongly is a question which need not concern us, for some Protococci are certainly plants. The Protococci are simple green cells which multiply by division into two, and so on, and which, at times, also produce in their interior zoöspores which escape and form new individuals.

How other chlorophyll-bearing plants might have arisen from Protococci we can not stop to consider, and we can only touch upon the possible origin of the colorless parasites. A vegetation consisting of simple forms like Protococci once established, there is no reason why there might not quickly have followed parasites of the order Chytridiaceæ the species of which abound at the present day in both salt and fresh water. The simple forms of the order consist of colorless cells which produce in their interior colorless zoöspores, which escape and attach themselves to submerged plants and animals.

The step from Protococcus to Chytridium is slight. We have only to suppose that a Protococcus has acquired the power of attaching itself to other Protococci or to low animals and has gradually lost the chlorophyll, which is no longer of service to a plant in a position to absorb nourishment directly from living organisms. Other natural changes would be the development of processes for attaching the Chytridium to the host or for enabling it to penetrate the walls of the host so that the parasite could make it's way into the interior. In short, it is probable that, at a very early epoch, true parasites existed essentially like our present

Chytridia. If the first plants were marine, it is altogether likely that the first parasites were Chytridia, if we can judge by present conditions. In the present age comparatively few species of fungi grow in salt water. The few that we have belong principally to the Chytridiaceæ and are abundant enough on the marine algæ of all groups. Most of the other marine fungi are forms like Leptothrix, which may rather be regarded as degenerate forms of Nostocs or Schizophyceæ than as forms derived from anything like Protococcus or Chytridium. It is certainly true that there are very few species of fungi higher than Chytridium or Leptothrix, if one can call Leptothrix a fungus, found on strictly marine plants. There are a few, however, and on the New England coast the stipes of the digitate Laminariæ, while yet submerged, are attacked by a species of Sphærella belonging to the Pyrenomycetes.

Whether the filamentous and higher forms of parasites have been derived from the simple Chytridia is not easy to surmise. Among existing Chytridiaceæ we have a series of genera in some of which there are simple rhizoids, and in others, like Cladochytrium, a well-developed mycelium. Furthermore, the species of, at least, three Cladochytria have lost the aquatic habit, and live in the tissues of Iris, Menyanthes and Sanicula. In Polyphagus, Nowakowski has also observed a conjugation of the mycelium of two individuals. Admitting the fugitive character of the mycelium of Chytridiaceæ, there is still no reason why the filamentous fungi might not have developed from species of this order. The zoöspore-bearing cells, as the parasite lost its aquatic habit and became aerial, might naturally be transformed into sporangia with non-motile spores, like those of Mucor, and, as it acquired the power of growing in solid tissues, one of the conjugating cells would advantageously be developed into a pollinodium, and we should then find oösporic forms. But it is hardly worth while continuing the chain of possibilities further in this direction.

As I have said, it seems to me not unreasonable to suppose that true parasites may have originated at a very remote period primarily from non-parasitic plants. But we must also consider another question. Is it not more probable that saprophytes were first developed and from them arose the true parasites? The line between saprophytes and true parasites is not well defined among existing plants. Some species might, with sufficiently good reason, be placed in either class, for what are called by Van Tieghem facultative parasites may live ordinarily as saprophytes and yet at times live a truly parasitic existence. The great majority of fungi are saprophytes, and De Bary has shown[7] in an instructive way how Peziza sclerotiorum, during a part of its existence, is a saprophyte, and becomes later a true parasite. The germinating spores will not penetrate the living cells of the carrot on which the mature forms of the fungus is found, but live a saprophytic existence for some time. After they have attained a certain growth and strength they are then able to make their way into the carrot, which they destroy. The mechanism is as follows: after a certain time the saprophytic hyphæ excrete an oxalate which is able to destroy the superficial cells of the carrot with which the hyphæ may come in contact, and the fungus then makes its way into the plant. It is probable that a considerable number of saprophytes may act in the same way as Peziza sclerotiorum, and it is not impossible that a good many existing saprophytes are developing into parasites, and, if the present state of things correctly represents what has always been going on, it would lead us to believe that the saprophytes first came into existence and the parasites followed. Since actual knowledge is out of the question, one can take either theory without denying the other *in toto*. The probabilities seem to me to favor the origin of Chytridia from Protococci, if we regard the morphological rather than the physiological side of the question. How far the first Chytridia were true parasites rather than saprophytes may be questioned. Decidedly, the majority of the living species, I should say, are parasites; but in some the parasitism is not well marked, and they may be conveniently called epiphytic.

Still another possibility must be considered. May we not suppose that the first living beings were protoplasmic bodies, neither plants nor animals, or both, if you please, and from them parasitic and non-parasitic plants were simultaneously developed? Orders like Myxomycetes might perhaps lead us to suppose that this view was the true one. But it may be assuming too much to suppose that Myxomycetes are plants at all. If they are plants, they have remained in a low condition, and have no offshoots represented by higher forms of plants. There appears to be only one way to find out whether a given structure is a plant or an animal, and that is to see whether it is described in zoölog-

[7] Bot. Zeit., 1886.

ical or botanical manuals. Unfortunately, this does not help us in the case of the Myxomycetes. We can safely say, however, that the more highly developed parasites have not been developed from Myxomycetes, and there is very little to lead us to believe that parasitic and non-parasitic plants were simultaneously developed from primitive protoplasmic structures.

It has already been stated that phænogamic parasites should be regarded as degenerate forms of other Phænogams. Their line of development is not through the parasites of the class of fungi. If one is willing to believe that the first parasites were Chytridiaceæ, or something very much like them, from which it is possible some of the filamentous zygosporic and oösporic fungi have been gradually developed, he is not, however, forced to believe that such a course of development is probable as well as possible. The class of fungi is not an homogeneous one. It is rather an assemblage of forms which have certain common physiological resemblances, but marked morphological differences. When one regards fungi as a single class of plants, and attempts to trace a clear connection between the highest and lowest members, he finds numerous gaps which can not well be filled. A general parallelism, however, exists between chlorophyll-bearing algæ and fungi, and one is forced to ask whether the order of development has not been from the lowest to the highest algæ—the class of algæ being more homogeneous than that of fungi — and whether the fungi have not arisen, not from any one primitive group of algæ, but from different groups of algæ at different periods in the progress from below upward. This view seems to be more in accord with existing facts than any other, and brings phænogamic parasites into harmony with the rest. If the phænogamic parasites may be regarded as derived directly from other Phænogams, so Chytridiaceæ may be supposed to be derived from Protococcaceæ. It may be that some of the zygosporic and oösporic fungi have come from the ultimate development of Chytridiaceæ, but it is more natural to suppose that the greater part of them are direct derivatives of zygosporic and oösporic algæ. Special applications of this theory would lead to so many technical details that they must be omitted on the present occasion. In general, if the theory is accepted, we should expect that the fungi first derived from any group of algæ would exhibit the characteristic modes of reproduction. In the sexual reproduction both groups are much alike, and if there are fungi at the present day whose reproduction is different from that of any algæ, it is because the reproduction has assumed more and more a non-sexual character, until, as in some groups of what are called higher fungi, sexuality has quite disappeared, as is supposed to be the case in Basidiomycetes. It is sometimes said that non-sexual modes of reproduction always precede the sexual. This is true only to a certain extent. It may be true, for instance, that in the earliest forms which had zoöspores the zoöspores were at first non-sexual, and afterward acquired the power of conjugating. But in fungi, where we have more non-sexual forms of reproduction than anywhere else, they must, in most cases, be regarded as secondary and degraded, not primary forms. Fungi are plants which depart more and more from what we may call typical plants. When we speak of higher plants we mean those in which the organs of assimilation and sexual reproduction exhibit a high degree of differentiation. When we speak of higher fungi, however, we refer to forms in which the vegetative organs are represented merely by a system of colorless threads, and in which the sexual reproduction is seldom well marked, if it exists at all, and they can be called high only in the sense that their numerous and often complicated modes of non-sexual reproduction are better developed than in what are called the lower fungi. In the struggle for existence among the higher plants those succeed best which are best able to assimilate crude material in the growing season and have the largest provision of seeds and reservoirs of assimilated food to carry them over the season of rest. In the struggle for existence among fungi, although there is an advantage if the mycelium is able to assume an indurated condition, like the sclerotia, at seasons unfavorable for growth, it is of much greater importance that there should be a variety of reproductive bodies, some of which, at least, are light and easily transported, while others are denser and better able to endure extremes of temperature and moisture, so that the fungus may be able to take advantage of any chance which may arise should the proper host be present. How well they are able to take advantage of temporary favorable conditions is shown in the rapid spread of epidemic diseases caused by fungi.

But it is better not to pursue the subject further. What I have already said will, I fear, appear to you too vague and uncertain; for the balancing of possibilities, although pardonable in philosophy, should not be carried too far in natural science. Of course, no celebration of our national anniversary is complete without a balloon ascension, and the more gas the better, provided the aeronaut, or, as the papers generally call him, the professor, only lands safely. So our society sends up its annual balloons in the shape of addresses in which the professors are allowed to soar above, though not out of sight of, facts. But they must not remain too long up in the air, and the gas for their balloons should be generated in the laboratory of experience and study. In their every-day work, it seems to me that the attitude of botanists, at the present day, is the correct one. Following the prevailing tendency in business affairs, the question they ask of plants is not so much, "Who is your father, and where did you come from?" as "What can you do?"

The identity of Podosphæra minor Howe and Microsphæra fulvofulcra Cooke.

MARTHA MERRY.

(WITH PLATE XI.)

About two years ago, while making a study of the American forms of the genus Microsphæra, the *M. fulvofulcra* Cooke came to my notice. The specimen examined was from the Ellis collection of North American fungi, No. 1,321. The so-called species is described in an article on "Californian Fungi," by Rev. J. E. Vize, in *Grevillea*, vol. v, p. 110, where it is said, "asci not seen." Examination of mature specimens shows clearly a single ascus in each perithecium, thus placing it in the genus Podosphæra. It agrees with the description of *Podosphæra minor* Howe, thus necessitating the cancellation of *Microsphæra fulvofulcra* Cooke.

Several specimens of the so-called Microsphæra fulvofulcra from different localities have been examined and compared with the original specimen of *Podosphæra minor* Howe. In all essential characteristics they agree. There are slight variations, arising probably from differences in hosts, vigor of growth, locality, age, etc.; but these variations are not marked. In many cases on the same leaf intermediate forms may be seen which unite the characteristics of the different specimens. They may all be embraced in the following description, a part of which is quoted from Mr. Howe's original description in *Bulletin Torrey Bot. Club*, v, p. 3: